XIII. *Electro-Physiological Researches.—Third Memoir. On Induced Contractions.* *By Signor* Carlo Matteucci, *Professor in the University of Pisa, &c. &c. Communicated by* W. Bowman, *Esq., F.R.S.*

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THE term *Induced* contractions was applied in England to a physiological fact discovered by myself, and described in the tenth chapter of my treatise on the Electro-physiological Phenomena of Animals. I shall henceforth adopt this denomination, since it has the advantage of expressing the phenomenon with brevity, and, to a certain degree, its nature.

I will begin by explaining, in a few words, in what this fact consists, together with the principal researches which I made in the commencement for the purpose of discovering its laws. Having prepared a galvanoscopic frog, I laid its nerve upon one or both the thighs of a frog prepared in the ordinary manner; this done, on applying the poles of a pile upon the lumbar plexuses of the frog, at the same time that the muscles of the thighs were contracted, contractions were excited in the galvanoscopic leg, the nerve of which reposed upon the thigh of the other frog. I discovered the same fact, placing the nerve of the galvanoscopic frog upon a muscle of the thigh of a rabbit, and exciting the muscle to contraction by means of a current which traverses its nerve. I have even seen contractions of the galvanoscopic frog occur without applying the electric current for the purpose of contracting the muscle which ought to induce the contractions, adopting for this purpose any other stimulus to the lumbar plexuses or to the spinal marrow. I finally tried these experiments, introducing between the nerve of the galvanoscopic frog and the inducing muscular surface very fine laminæ of different substances. A leaf of gold and a very fine non-conducting stratum of mica or of glazed paper being interposed prevented the phenomenon, that is to say, the induced contractions in the galvanoscopic frog failed to appear, whilst a stratum of fine paper soaked in water did not interrupt the induced contraction. From the whole of these facts I was led to conclude,—1st, that the contraction induced in the galvanoscopic frog could not be attributed to the effect of derived currents; 2nd, that it should rather be considered the effect of an electric discharge taking place during the contraction of a muscle. For the sake of supporting this explanation of the induced contractions by facts, I instituted a great number of experiments which are described in the tenth chapter above referred to. With this view I composed a pile of entire frogs, and closed the circuit with the two extremities of the galvanometer. Allowing the needle to become stationary, I touched specially the nerves of the frogs composing the pile with a solution of potassa, by which means

contractions were excited in these frogs. Operating in this manner, I have often remarked the deflection of the needle to be increased by a few degrees, after which the needle retrograded. When the frogs were touched several times with the potassa, or were very much weakened, so that touching them again with the alkali no longer produced contractions, it has, in most cases, occurred that there was no sign of increased deflection in the needle of the galvanometer. Finally, bathing the nerves of frogs arranged in piles with acid or saline solutions, the deflection, far from increasing, rapidly diminished, at least in the beginning.

These facts, with which I paused, might have appeared in some manner favourable to the idea that the induced contractions were the effect of an electric discharge which accompanies the act of muscular contraction: notwithstanding this I terminated the chapter referred to with the following words:—"I cannot take upon me to affirm that the question is entirely solved, and I pause here from not knowing how or by what way to advance to solve it."

The importance, however, of the fact of induced contractions thus always appeared to me very great, and consequently I have not failed to give my strictest attention to the study of it, and I have reason to believe, latterly, with some success. I shall, therefore minutely describe in this memoir all the experiments that I have instituted upon the induced contractions, and I trust the reader will excuse the prolixity of the description.

Before commencing a fresh series of investigations into the fundamental fact of the induced contractions, I thought it necessary to repeat and vary the experiments of which I have already given an outline, and which were directed to the purpose of discovering whether there is a development of electricity during the contraction of a muscle. In order to have a more fixed and considerable deflection, it was necessary to employ piles consisting of a greater number of elements than those I had made use of previously. I imagined that for this purpose a muscular pile would be far preferable to a pile of frogs. I avail myself of this opportunity of referring to an experiment made for the purpose of proving the existence of the muscular current in the living human subject. I applied the nerve of the galvanoscopic frog with care to the muscle of a leg laid bare by a wound. The most lively contractions were excited in the galvanoscopic frog every time that the circuit was suitably closed between the interior of the wound and the surface.

My late experiments have shown beyond all doubt that, the number of elements taken from the same frogs being equal, the muscular current is much stronger than the proper current. I have recently shown that when from defect of nutrition, a very low temperature, or the action of sulphuretted hydrogen, &c., both the muscular and the proper current of the frog are weakened, the diminution is much more considerable in the latter than in the former. And in fact, composing the pile described at p. 116 of my treatise with halves of frogs prepared by cutting the thighs in half, I find a differential current varying in intensity, but always in the direction of the muscular current. It is only in very robust frogs, dividing the thigh in the thickest

part, and leaving a very small internal surface of the muscle exposed, that the differential current is found to be either null, or in the direction of the proper current. This fact attracted my attention in the course of my early experiments, but the explanation of it is clearer to me since my later investigations, reflecting that in leaving the thigh almost entire, we have two elements, that is to say, the muscles of the leg, and those of the thigh, which direct the current in the same direction, while there is but one element of the muscular current which gives a current in a contrary direction.

Returning to the subject of this memoir, I would observe that I have employed a muscular pile in the view of determining whether there is evolution of electricity in the contraction of a muscle. But since in order to excite the contractions of the muscle, I was obliged to bathe it with concentrated saline or acid solutions, or better with alkaline solutions, I previously studied the effects of the action of these liquids upon the muscular current. In this view, from a great number of frogs I selected eight which I prepared in the usual manner, and which furnished sixteen elements or half thighs. Closing the circuit, the needle reached 90° and remained stationary at 22°. Constructing another similar pile, after having well-washed and then dried the sixteen half thighs, the result was the same. Another sixteen similar elements were immersed for a few seconds in a diluted solution of sulphuric acid, and afterwards washed several times in water, so that they did not redden turnsol paper. Composing the pile and closing the circuit, a current was produced in the direction of the muscular current, but the deflection was only 6° or 7° at first, and the needle was fixed at 0°. I rapidly cut the half thighs with a pair of scissors so as to renew the internal surface of the muscle in a fresh state. Recomposing the pile, the current was still weaker than that indicated above. Thinking that the effect of the acid solution upon the muscular elements was to diminish the conductibility, I constructed a muscular pile of eight half thighs of frogs taken from frogs previously intact, to which I added four entire thighs taken from frogs likewise intact: the current which resulted was 46°. In the room of the four entire thighs, I next substituted four entire thighs which had been immersed in sulphuric acid and then washed, the current was 44°. The conducting power therefore had not varied in the muscular masses subjected to the action of the acid solution. To be still more certain of this conclusion, I tried the experiment already described, substituting eight half thighs for the four entire ones for the purpose of prolonging the circuit. These eight half thighs being treated with the acid solution, and joined together by contact of the internal surface of one with the external surface of the other, the result was the same.

I next repeated the same experiments, using a sufficiently concentrated solution of potassa in which to immerse, for a few seconds, the muscular elements or half thighs. These elements were then washed in pure water until they showed no alkaline reaction. Composing the pile with sixteen elements, and closing the circuit, there was a first deflection of from 10° to 12° in the usual direction of the muscular current,

and an imperceptible stationary deviation. Making a fresh section of the muscle and recomposing the pile, the result was the same. And in this case, likewise, the conductibility was not changed; consequently the alkaline or acid solutions act as I had before found water act at a high temperature. I will here repeat an experiment which I made, merely to show its accordance with those related above. Sixteen half thighs of frogs were immersed for a few seconds in water at about 50° C. On taking these elements out of the warm water and bathing them with cold water, I constructed a pile, closed the circuit, and had a first deflection of 12° in the direction of the muscular current, and the needle stopped at 0°. After having renewed the internal surface of the muscle by making a fresh section, I recomposed the pile, and the signs of the current were the same as before. And also in this case I assured myself of the fact, that the conductibility had not been sensibly changed by the action of the hot water. I will add, moreover, that it is not to the repeated washing in pure water at the ordinary temperature, that the diminution in the intensity of the muscular current is to be attributed. I have very frequently seen the same deflection slightly varying in intensity, produced by a pile of a given number of elements, or half thighs, sometimes washed in pure water, at others not washed at all. Even a solution of chloride of sodium highly concentrated, is capable of diminishing considerably the signs of the current produced by a pile of which the elements have been immersed for some seconds. Thus, whilst sixteen common elements produce a first deflection which mounts to 90°, and remains fixed at from 20° to 22°, if these elements be left for a few seconds in the saturated solution of the sal marinus and then taken out, the first deflection is about 60°, and the needle becomes stationary at between 8° and 10°.

We are thus led to conclude, that by the action of the alkaline acid or saline solutions in a concentrated state, those conditions of the muscular elements by which the evolution of electricity takes place, are destroyed. Nor is this conclusion in opposition to the admitted origin of this current: but since by the action of the acid, or saline solutions, the signs of the muscular current either cease or are greatly weakened, it remains to be explained why, in the experiments reported in my work, and of which I have given an outline in the commencement of this memoir, the diminution of the current did not take place on touching the elements of a pile of frogs with an alkaline solution, while it occurred immediately on touching them with an acid solution.

On the contrary, we have observed, that operating with alkalies, at the first contractions that are excited, there is in many cases a perceptible increase of deflection, which lasts for some seconds. With acids, on the contrary, the deviation is lessened immediately, and returns again after a short time.

Before endeavouring to account for these phenomena, I will describe those of my experiments performed with the greatest exactitude, in the view of ascertaining whether there be in contraction any evolution of electricity. I prepared a great number of frogs according to GALVANI's method; I then cut their legs, disarticulating them

as well as I was able: there remained a couple of thighs united by a portion of the spinal chord. One of these thighs I cut in half, and in this manner I had a certain number of elements all alike and consisting of an entire thigh, a portion of the spinal chord, and a half thigh. It is easy to understand how, with these elements, I composed a muscular pile, that is to say, applying the external surface of the entire thigh upon the internal surface of the thigh of the succeeding element (fig. 13.). This done, I immerse the extremities of the galvanometer in the liquid in which the extremities of this pile terminate. A small appendage to the extremities of the conducting wires of the galvanometer, precludes the necessity of my holding the latter with my hands when I require to complete the circle. I have repeated this experiment a great number of times, at one time using a pile of twelve, at another of sixteen, at other times of twenty elements. Both the first deflection, and that at which the needle remained stationary, were somewhat weaker than they would have been had the piles been composed of an equal number of halves of thighs only. This difference is mainly attributable to the greater length or resistance of the circuit. In every case referred to above, after having suffered the needle to become stationary, which it did in the various experiments at 10°, 12°, and sometimes 15°, I touched the lumbar plexuses of the different elements with a sufficiently concentrated solution of potassa, excepting, however, the two extreme elements, for fear the alkaline solution should reach the liquid in which the extremities of the conducting wire of the galvanometer were immersed. The muscular contractions took place immediately upon the application of the alkali, and lasted for some seconds without ever being sufficiently strong to interrupt the pile by displacing the elements. During these contractions the needle of the galvanometer did not move. In some cases I have seen the needle go back, in others rise to 2° or 3°. But these variations are too uncertain in the greater number of cases, and most generally correspond to a too sudden motion of the elements of the pile which disturbs their contact.

Let us then conclude that direct experiment answers negatively to the question we proposed to solve, whether there were evolution of electricity in muscular contraction.

Having got rid of this part of the question, the next which presents itself is the consideration of the phenomena which we observe in the proper current (for which we employ whole frogs), and which consist in signs of increased intensity on first touching the lumbar plexuses of the frogs with potassa, while, on the contrary, an acid solution causes the needle to fall instantaneously. To arrive at a knowledge of these facts I have repeated and varied my former experiments, and the explanation of the facts is as follows:—

Whatever be the form of the elements used for constructing the muscular pile, that is to say, whether it be made of entire frogs, of halves of thighs, or, as described (fig. 13.), if the surface of the muscular elements be bathed with an acid or alkaline solution, it constantly happens, whether there be contractions or not, that the de-

flection diminishes and the needle returns to zero, where it remains if the action of the alkali be repeated, or the solution too concentrated. This effect is identical with that already described, which occurs in the muscular elements immersed for a few seconds in acid or alkaline solutions. In the manner of conducting the experiment (fig. 13.) which we have adopted, in order to excite the muscles to contraction, we touch with the alkali such points of the muscles as are in a certain manner out of the circuit, and which certainly do not constitute the electromotor part of the element. In the pile of entire frogs, with which we succeed oftenest in obtaining signs of increase of the current for a few seconds, on touching the lumbar plexuses alone with the alkali, these signs of increased intensity never occur if the entire muscular surface be bathed with an alkaline solution. I will add, moreover, that if an acid solution be employed, and care be taken to touch the lumbar plexuses only with a paint-brush, carefully avoiding the muscles of the thighs or legs, the deflection is not diminished, and in spite of the contractions which are excited, although these are less than those which the alkali occasions applied upon the muscles, there is no increased deflection. The surface of the muscle must be touched with the acid to make the needle fall. The same occurs with the alkali, and is, I repeat, in accordance with the experiments already referred to with the muscular elements which have been immersed in the acid or alkaline solutions.

It is then only with the pile of entire frogs, and only when the lumbar plexuses of these alone are touched with the alkali, that sometimes a slight increase of deflection is perceived, while this does not occur when acids are similarly applied. Taking up our ground upon the strength of all the experiments described above, it is impossible to consider this result as contrary to the absolute answer in the negative given above to the question whether there were development of electricity in muscular contraction.

In the course of the present memoir other irrefragable proofs will be adduced in support of the assertion to the contrary. It is impossible for anybody who looks well to the whole of these phenomena, not to perceive the difficulty which occurs in endeavouring to explain why, in the particular case described above, the alkali should produce an increase of deflection in the proper current of the pile of entire frogs. For myself, I incline to the belief that stronger and more permanent contractions being excited by the alkali than by acids, the contact between one element and the other becomes thereby more intimately established in the greater number of cases, and, by that means, the internal conducting power is increased. In effect, this contact is very imperfectly established in the pile of entire frogs, and very great difference in the intensity of the current is perceptible with the same elements by improving their mutual contact.

Whatever may be the right interpretation of the slight increase which occurs in the intensity of the proper current, on touching the lumbar plexuses of the frogs, and thus exciting muscular contractions, certainly this fact alone is insufficient to

lead us to admit the evolution of electricity during muscular contraction, while, as certainly, all the other cases described oblige us to conclude the contrary.

I will now proceed to detail several other new researches upon the phenomenon of induced contractions: but I must first entreat that the reader will excuse my trespassing on his time with a lengthened account of my numerous experiments: the fact of induced contractions is certainly of such importance, and at the same time so obscure, that it cannot be established except by long and patient researches.

No one who has once seen induced contractions occasioned by contractions excited by other means than an electric current, can hesitate to admit that this current is not the direct cause which produces them.

If the nerve of the galvanoscopic frog be applied upon the muscles of the thigh of a frog prepared in the ordinary manner, and the spinal chord be suddenly lacerated with scissors, or with the broken edge of a piece of glass, or by any other means, induced contractions very rarely fail to occur. It is however certain that passing an electric current through the lumbar plexuses, the phenomenon of induced contractions scarcely ever fails.

As I have most frequently resorted to the passage of the electric current for exciting contractions, I have taken every possible precaution to prevent any portion of the electric current from invading the galvanoscopic frog or the thighs of the entire frog. The best mode of conducting the experiment, and the way which offers the best chance of success, is to fill a common dinner-plate with Venice turpentine and spread the frog upon it. It is hardly necessary to say that the turpentine should be so dense that the frog cannot sink in it; care must be had in preparing the galvanoscopic frog not to leave any portion of muscle attached to the nerve.

Whatever may be the position of the nerve of the galvanoscopic frog relatively to the muscular fibres of the thigh on which it is laid, the phenomenon of the induced contractions always continues. Thus on some occasions I have extended this nerve parallel to the muscular fibres, or I have extended it normally to the said fibres, or in fine, I have bent it in a zigzag, that is, in all directions, and the induced contractions have been obtained in every case and without sensible difference.

These induced contractions are obtained by applying the nerve of the galvanoscopic frog on the gastrocnemius muscle of the leg.

I have also attempted, by washing the galvanoscopic frog several times in pure water, to remove any trace of blood or other humours which might be sprinkled over the surface of its muscles, and the induced contractions have equally continued.

I have cut with a razor, or better still with a pair of scissors, the surface of the muscles; I have then placed the nerve of the galvanoscopic frog upon the cut surface only of the muscles themselves; the induced contraction has taken place. It has also occurred on disposing the nerve of the galvanoscopic frog upon the muscle so that the extremity of the nerve should fold back over the nerve itself, and thus form a kind of closed circuit.

I have also wished to ascertain if the induced contractions continued even when the nerve of the galvanoscopic frog had not been cut. I have therefore prepared the frog, so as to preserve the nerve entire, as follows. Having skinned a frog I remove the viscera, then the bones and muscles of the pelvis, and finally the muscles of the thigh, taking care to preserve the nerve of the thigh. In this manner I obtain a frog whose nervous system is entire, and which has a long nervous filament uncovered, that is, the lumbar plexus and the nerve of the thigh. Having thus obtained the frog, I prepare another in the ordinary manner, which I place upon the turpentine in the manner already described. Then I place the nerve of the frog, prepared as has been said, upon the thighs of the other frog (fig. 14.). On exciting the muscular contractions, the induced contractions are obtained as they are by using the galvanoscopic frog alone; and at the same time the contractions in the muscles of the back and in the other leg are obtained. We shall have occasion to recur to this experiment further on; we now limit ourselves to deduce from it that the induced contractions are obtained, even when the nerve placed upon the muscles in contraction is entire.

In using the frog thus prepared, I have experimented upon the induced contractions, by causing the nerve that is in contact with the muscle in contraction to be already in some manner excited by a current or some stimulus. For this purpose I have comprehended the galvanoscopic frog in the circuit of a voltaic circle, or have applied upon the nerve a drop of an alkaline solution. Every time that the inducing muscles are contracted, there is always induced contraction, whether the nerve through which this current is transmitted be already excited or not, and consequently, even when the muscles in which the induced contractions are generated, are already in contraction; and, in fact, in spite of the contraction already present there is no difficulty in perceiving the induced contraction that follows.

Many easy experiments may be made to prove that in whatever way the nerve of the inducing muscle be excited, if its contraction fail, the induced contraction is also wanting. I shall limit myself to reporting some of the principal ones. Having cut the nerves at two or three points in the interior of the inducing muscle, so as to prevent its contracting, the induced contraction is wanting when the nerve is in any way stimulated external to the inducing muscle.

If, without cutting the nerve, all the tendinous extremities of the muscles of the thigh are severed, and transverse cuts are also made in those muscles, taking care not to divide the nerves, on stimulating them, the inducing and also the induced contractions are wanting.

By removing with care all the muscles of the leg of a frog, the nervous filament that runs in the leg itself may be uncovered. This nerve may be irritated either with the current or any other stimulus, after having extended the nerve of the galvanoscopic frog upon the muscles of the thigh above. These muscles of the thigh do not contract; the induced contraction is wanting.

By operating upon rabbits or upon dogs, I have been able, with the electric current, to act upon the nervous filaments that run to the kidneys, to the stomach, and to the intestines: when the nerve of the galvanoscopic frog was extended upon the different parts in the same conditions as for the muscles, I never obtained any sign of induced contraction.

I have also sought to discover if there was contraction induced by applying the nerve of the galvanoscopic frog upon the excited nerve. For this purpose it is sufficient to prepare two galvanoscopic frogs, and to extend the nerve of the one upon the nerve of the other, in the points nearest to the leg. To perform the experiment with every care, the two frogs are disposed upon turpentine. Then, either with the current or with some other stimulant, the superior points of the nerve of the frog, that I shall continue to call inducing, are erected. There is no induced contraction in the galvanoscopic frog, although this contraction immediately occurs, if its nerve is extended over the gastrocnemius of the other. It is needless to say that in using the current to excite the inducing contraction we must never place either of the electrodes of the pile in contact, or in proximity to the nerve of the galvanoscopic frog.

It is proved by the above experiment that an excited nerve, and one in which is certainly propagated that cause, whatsoever it be, which awakens contraction in the muscle and sensation in the brain, does not act upon the nerve of the galvanoscopic frog placed in contact with it. I will also add the following experiment. I have uncovered the brain of a frog prepared in the ordinary method with the greatest possible care, and I have extended upon it the nerve of the galvanoscopic frog.

In various experiments thus tried, I have applied the current sometimes direct, sometimes inverse, upon the lumbar plexuses, and in others I have touched these plexuses with potassa, and I always obtain the contractions in the lower limbs and convulsions in the back. However, I have never found signs of induced contractions in the galvanoscopic frog that was extended upon the brain. The induced contractions are therefore originated solely by the muscle in contraction.

I have sought to discover how these induced contractions grow weak, by causing them to be originated by means of a muscle whose own contraction was induced. In a word, I have examined the induced contractions of the second and third order, &c. For this purpose I prepare various galvanoscopic frogs, and one in the ordinary manner, and I dispose them in the following way. Upon the muscles of the thighs of the entire frog I extend the nerve of a galvanoscopic frog: upon the gastrocnemius of this, I extend the nerve of another galvanoscopic frog, and so on in succession. The whole is placed upon turpentine. On exciting the contractions of the entire frog by making the current pass through its lumbar plexuses, I have seen in many instances *three* galvanoscopic frogs contract, and all with nearly the same vivacity. The contractions are never wanting in two frogs, but I have never been able to perceive four contracted. There is therefore an induced contraction of the first, the second, and the third rank. Before coming to the consequences to be drawn from the above-

mentioned facts, it remains to me to describe the many experiments I have made for the purpose of discovering the influence of bodies interposed between the muscle in contraction and the nerve of the galvanoscopic frog upon the induced contraction. From my first experiments upon the induced contraction, I had perceived that on extending a sheet of gold leaf, such as is used for gilding, upon the muscle, and then placing the nerve of the galvanoscopic frog upon the gilded muscle, the induced contraction did not take place. That this should happen, it was necessary that the muscle should be completely coated with the gold leaf, which is not the case after one or two contractions when the gold leaf gets torn. I had then seen that a varnished paper (*papier glacé*) interposed between the muscle and the nerve impeded the induced contraction; and lastly, a sheet of felt soaked with water or the serous liquid that bathes the surface of muscles, and interposed between the muscle and the nerve of the galvanoscopic frog, does not prevent the induced contractions. Our knowledge was limited to these three cases relative to the action of interposed bodies upon the induced contraction. I have therefore sought to extend and vary the experiments. The manner of operating that I have adopted, consists in preparing a frog in Galvani's method and placing it upon turpentine; while an assistant is preparing more galvanoscopic frogs whose nerves I extend upon the muscles of the thighs of the first frog. In order to awaken the inducing contraction, I always use a small Faraday's pile of fifteen elements immersed in pure water, and whose electrodes are covered with silk and varnished.

There is no liquid body among the many examined that impedes the induced contraction; pure water, slightly acidulated and saline water, serum, blood, olive oil, diluted alcohol, the varnish of alcohol and resin, volatile oil of turpentine are the liquids made use of in these experiments, and through which the induced contraction takes place. I am always accustomed to let some drops of the liquid under experiment fall upon the muscle, and to dip the nerve of the galvanoscopic frog in the same liquid. The induced contraction still subsists even if a thin sheet of felt imbued with the above-mentioned liquids is interposed between the muscle and the nerve.

The slight conductibility of some of the liquids made use of (oil, oil of turpentine, varnish, &c.) has made me doubt whether the induced contraction would not subsist even in spite of the interposition of an absolutely insulating body. I assured myself, in fact, that across a layer, even very thin, of the said liquids, neither the muscular current nor the proper one was propagated. On holding the galvanoscopic frog in the hand, and causing its nerve to come into contact with a wetted paper that in any manner communicates with the ground, the contraction, as is well-known, is obtained. The same thing happens on touching with the nerve of the galvanoscopic frog, the muscles either of a frog or of any other animal in communication with the earth. In all these cases it is always the proper current that circulates through the observer, the ground, the touched body, and the galvanoscopic frog. Now if we wet the nerve of the galvanoscopic frog either in common oil, or in oil of turpentine, or in varnish,

the thin stratum that adheres to the muscle is sufficient to impede the circulation of the proper current.

It is therefore indubitable that if an induced contraction is propagated through a stratum of the bad conductors mentioned, this induced contraction cannot possibly be owing to a current generated in the contracting muscle, and passing thence into the nerve of the galvanoscopic frog.

Nevertheless these experiments were so important for the theory of the phenomenon of induced contraction, that I desired to try the effect of interposing between the contracting muscle, and the nerve of the galvanoscopic frog, a still worse conducting body than those mentioned. The body that has served me in these experiments has been Venice turpentine nearly solid, and rendered more or less liquid by adding to it a little volatile oil of turpentine. Having smeared over the thigh of a frog with this mixture, and wetted the nerve of a galvanoscopic frog with it, I prepare the experiment as usual, and the induced contraction continues. To prove the bad conducting powers of the mixture made use of, I hasten to say, that if I apply one pole of the pile with which I excite the contractions upon the stratum of the insulating mixture, of course without penetrating to the muscle, and I touch with the other pole the nerve of the galvanoscopic frog, the contractions are not excited in it. It is therefore proved from this experiment that the induced contraction propagates itself through a stratum of an insulating substance that prevents the propagation not only of the muscular and proper currents, but also of that current which excites the inducing contraction.

If the insulating stratum exceeds certain limits of thickness, and the mixture has not a convenient degree of fluidity, the induced contraction is wanting. It is however impossible to determine within what limits of thickness in the stratum and fluidity in the mixture this occurs; it is sufficient for me to have established by experiment that in some cases the induced contractions are obtained, while there is interposed between the nerve and the muscle an insulating stratum which certainly arrests the muscular and proper current, no less than an ordinary voltaic current.

I shall say, finally, that I have never succeeded in obtaining the induced contraction when using a solid body interposed, however thin it might have been chosen, and whatever might be its nature. For this purpose I used flakes of mica extremely thin, flakes of sulphate of lime, gold leaf, paper smeared with glue, and leaves of vegetables. The induced contraction is always wanting. It is however a very curious, and I believe even important fact in its consequences, to obtain the induced contraction through the skin of the muscles of the inducing frog. The experiment never fails of success, whether the inducing contraction be excited by the electric current, or by any stimulus applied to the lumbar plexuses of the inducing frog.

Having thus described a long series of facts relative to the circumstances that intervene in producing, in modifying, and in destroying the phenomenon of induced contraction, it might be believed that with the aid of these I might ascend to the physical theory of the phenomenon.

Unfortunately I am rather doubtful of it, and in this uncertainty I again beg the reader to follow me in the discussions that I shall be compelled to make upon the different hypotheses that may be imagined by which to interpret the phenomenon of induced contraction.

I. It is sufficient to have once seen the induced contraction originated by arousing the inducing contraction by any mechanical stimulus, to no longer have any kind of suspicion that the electric current used for exciting the contraction is propagated to the nerve of the galvanoscopic frog*. How must we understand the induced contraction of the second and third order? How are we to explain the fact that the induced contraction is wanting (although the current may be applied as usual upon the lumbar plexuses of the inducing frog), only because by the section of the nerves in the thigh the inducing contraction is prevented, or at least greatly weakened? Why is the induced contraction wanting when we apply the same current upon the nerves below the thigh in which there are no inducing contractions? Why, when we act with a current upon the lumbar plexuses of a frog already weakened so as no longer to have the contractions except at the beginning of the direct current or the cessation of the inverse, why in these cases alone is there induced contraction?

It is useless to continue to enumerate the objections that may be made to the interpretation of induced contraction by referring to the diffusion of the current exciting the inducing contractions, a diffusion that can in no way be physically conceived.

II. It might be suspected that the induced contraction was the result of a mechanical stimulus—of the shock of the inducing muscles, which, in their contraction, shake the nerve of the galvanoscopic frog. I have attempted many times (using extremely delicate galvanoscopic frogs) to produce motion in the muscular masses of the thighs in every possible way, and I never could see the galvanoscopic frog contract itself. If the occasion of the phenomenon were this shock, how could we explain the cessation of the induced contraction by the interposition of a very thin sheet of gold leaf or mica between the nerve and the muscle?

I have very many times tried to apply the nerve of the galvanoscopic frog upon plates of metal and glass, upon stretched membranes, upon cords of catgut while they were vibrating, and there was never any sign of contraction in the galvanoscopic frog. It is not then the shock of the muscle in contraction against the nerve of the galvanoscopic frog that occasions the induced contraction.

III. It happens very rarely that the contraction in the galvanoscopic frog is obtained when the nerve is being stretched over the thigh of the other frog, and this even when both are perfectly insulated. It is however certain that every time that this happens the occasion does not fail to be discovered. It either consists in

* Through excess of caution I have many times endeavoured to obtain the induced contraction by exciting the inducing contraction from the laceration of the spinal marrow by means of a fragment of glass; the induced contraction has taken place as if the inducing contraction had been excited by the current, or by any other stimulus.

the inside of the muscle being uncovered in some points, or because the nerve of the galvanoscopic frog remains united to some piece of muscle, that folding back again touches the nerve when this is extended over the thigh. It has also appeared to me that sometimes these contractions occur when the tendinous extremities and the surface of the muscle and of the thigh touch two points of the nerve of the galvanoscopic frog. So let us say that the induced contraction takes place constantly in all cases in which, by the care taken, the above-mentioned circumstances that may awake the contraction of the galvanoscopic frog are verified. We know too that having cut with scissors the muscular superficies of the thighs, and rendered them thus quite uniform, the induced contraction continues, when the nerve of the galvanoscopic frog is applied upon the new internal surface of the muscle. This induced contraction subsists even through the skin of the frog, and on interposing insulating liquid layers between the nerve and the muscle. We have seen that the insulating power of those layers was such as not to permit the circulation of the proper and muscular currents. How then can it be supposed that the induced contraction should take its origin from the above stated circumstances, even admitting that they may be rendered more active, or that they may be excited by the muscular contraction? These circumstances reduce themselves to the phenomenon of a muscular current or of a proper current, which ought to traverse the nerve of the galvanoscopic frog, while that nerve would be enveloped by a stratum of an insulating substance, which we have seen cannot be.

IV. The first idea conceived by which to interpret the induced contraction, was that of an evolution of electricity which might accompany the muscular contraction. There is evolution of heat in the act of contraction; and, according to the important observation of Quatrefage, which there is much necessity for repeating, in order to exactly establish its details, there would appear to be development of light in certain cases of muscular contraction; so that analogy might lead us to infer the probability of the production of electricity by the muscular current: besides, the few experiments that I made when I discovered the induced contraction might very well be interpreted on this hypothesis. An insulating body, as a lamina of mica or varnished paper, when interposed, impeded the induced contraction: and it could not be otherwise. The same thing occurred even when a lamina of gold perfectly discharging the electricity, that it is supposed is produced by the contraction, prevented the nerve from being traversed by it.

In spite of these first steps, which were flattering me into giving a very simple explanation of the induced contraction and were leading me at the same time to prove an important phenomenon in the muscular contraction, I am now constrained to entirely abandon this idea because it is contradicted by experiment.

In the beginning of this memoir, I referred at all possible length to the many experiments made by which to examine if there is any augmentation of the muscular current, or of the proper one in the act of contraction. All my efforts have been useless, and I have been obliged to conclude that experience does not prove that the signs of the muscular or proper currents increase in the act of muscular contraction.

We might believe it to be owing to an evolution of electricity independent of the muscular and proper current; but how can we suppose it so, when we see that the induced contraction propagates itself through certain insulating strata, as turpentine, oil, &c., while it does not take place if we use an extremely thin plate of mica? It might be supposed that the electricity developed in the muscular contraction could have acted by induction. In this hypothesis it is clear why the turpentine does not stop the induced contraction, but it still remains doubly obscure why with the extremely thin plate of mica this should take place. I have made an experiment by covering a galvanoscopic frog, placed upon a plate of glass, with a sheet of mica; the electric discharge of a jar passes between the knobs of the universal discharger upon the sheet of mica, and the contractions in the galvanoscopic frog are aroused. I shall not occupy myself at this moment in analysing these facts; it is sufficient at present to prove that there must be induced contractions through the plate of mica if the occasion of the phenomenon were an electric discharge. I will add finally, that I have very many times attempted, and always uselessly, to awaken the contractions in the frog, by holding the nerve of the galvanoscopic frog near and almost in contact with a metallic conductor traversed by the electric current.

To place myself in favourable circumstances that the induced current may be complete in the frog, I prepare it in such a manner that a long nervous filament (that is, one of the lumbar plexuses with its continuation in the thigh) may be uncovered. The frog is otherwise intact, and the two legs touch each other. I support the frog with silken threads, so that it may be horizontal, and that its nervous filament may be in contact and parallel with the voltaic conductor which is varnished. When every care is taken to insulate the frog, signs of contractions are never seen in it either at the closing or the opening of the circle of the pile. It is seen that by this disposition the induced circuit may take place in the frog. I have used a BUNSEN's pile of ten elements without any result.

There is therefore given no experimental proof of that explanation of the phenomenon of induced contraction which admits an evolution of electricity in the act of muscular contraction.

We are still ignorant of the cause of muscular contraction, and we know nothing of this phenomenon except that it occurs on acting even at a great distance from the muscle upon the nerve that ramifies within it; that this action requires for its propagation the integrity of the nervous filament from the point at which it is acted upon to the muscle; that this propagation acts with a velocity which we cannot judge to be less than that of light and heat and electricity in their different media; that that which modifies, augments, or destroys the complication of the physico-chemical phenomena comprehended in the nutrition of the muscles, operates equally upon its contractility under any stimulus affecting the nerves; finally, that the phenomenon of the contraction of a muscle ought to be understood, with the condition that whatever be the cause of this phenomenon, it acts in accordance with the physical law of

elastic bodies. The phenomenon of induced contraction would seem to be a first fact of induction of that force which circulates in the nerves and which arouses muscular contraction.

Admitting that we cannot give a satisfactory explanation of the phenomenon of induced contraction by recurring to electricity or any other known causes, as I think I have abundantly proved, it appears to me that we cannot, confining ourselves to a first fact, as is that of induced contraction, interpret it differently from what we have done. The induced contraction is only a new phenomenon of nervous force, a phenomenon of which we have given the principal laws in this memoir. It seems to me therefore more just to call that henceforth *muscular induction*, which I have hitherto called induced contraction. I shall conclude this memoir with some applications of muscular induction to physiology.

By the experiment described above (fig. 14.), it is proved that the muscular induction is propagated in a nerve at the same time towards the two extremities—towards the muscle as well as towards the nervous centre. If the muscular induction not only acts upon the nerve in contact with the muscle but also through some interposed bodies, it is natural to admit that when a muscular mass enters into contraction by the irritation acting on one of its nerves, the phenomenon of induction should occur in all the other nerves. And even wishing to yield for a moment to the analogies that exist between the electric current and the nervous force, we might be led to believe that this induction should take place upon the excited nerve which is the cause of the contraction. Could we not perhaps from this deduce a physical explanation of a well-established physiological fact, that within certain limits the activity of the muscles increases in proportion as the contraction is aroused in them?

It also appears to me that a great number of those movements which occur in us and in animals independently of the will, but yet following others occasioned by the will, may be considered as phenomena of muscular induction. I leave to physiologists the continuation of these studies, which appear to me worthy of all their interest.

I shall terminate by citing an experiment which I think proves an action of this nature. Having prepared a frog in the ordinary manner, I cut one of the nerves that constitute one of the lumbar plexuses, and I divide it precisely at the point of exit from the vertebral column. Having extended the frog upon turpentine, I draw on one side the severed nerve, and I irritate it either with the current or with an alkali. Thus very strong contractions are produced in the thigh, and at the same time, if the frog is very lively, there are contortions in the back and movements in its superior limbs.